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SUBJECTIVITY IN *EX ANTE* RESEARCH EVALUATION

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Abstract

This essay is a critique of research evaluation research. Considerable evidence exists that agricultural research conducted during the era when projects were chosen by diffuse selection systems yielded extraordinarily high returns. It is not obvious that the formalized, quantitative, and typically centralized selection models can be expected to produce higher contemporary returns than the decentralized informal mechanisms. All *ex ante* evaluations are intrinsically subjective, regardless of technique used to generate the evaluation. The extreme uncertainty surrounding the nonrepetitive new-knowledge production function further limits the potential of the sophisticated selection procedures. Perhaps of greatest importance, however, are the high costs imposed by these procedures in terms of scientists' time, morale, and "artistic" research tool atrophication.

Purpose of Research Evaluation

Much effort in recent years has been invested in devising new methodologies for evaluating public agricultural research. Reasons for wanting to evaluate research are many and varied. They range from measurement of the historical rate of return for research investments to assessment of the influence of various organizational participants on research selection and conduct. However, just as the many and diverse intermediate objectives of economic research ultimately funnel into the overriding end goal of improving predictive performance, so the major objective of research evaluation research condenses to improving predictions of costs and benefits of future research. The ultimate practical objective of all this work is purely *ex ante*, i.e., to provide relevant information for future funding decisions. Determining the productivity of past investments is mainly academic as they are sunk costs. Their relevance must be measured by the extent to which they are useful for making variable cost (i.e., present and future) decisions.

Do We Need Formal *Ex Ante* Analysis?

Ex ante evaluation of research alternatives is not a new concept. It has always been conducted in some fashion at one or more levels in the research organizational hierarchy. Historically, the major assessment of project alternatives has been made by the individual scientist acting as an entrepreneur on behalf of his own professional life. Administrators have reviewed proposals submitted by scientists and approved, disapproved, or modified them, but the major administrative roles have been more in trying to increase total funding and in selecting scientists who would be with the organization for extended periods than in selecting individual projects. Of course, the role of administation varies from organization to organization, but few have implemented formal, systematic evaluation procedures for quantitatively measuring the worth of one research area against another.

The justification for proposing a change in the way *ex ante* research evaluation is conducted must ultimately rest on one of the following perceptions: (1) the current system is not working well, or (2) some evidence exists that even though the current system is working well, it could do significantly better with a change.1/ Let's investigate brifely whether there are sufficient grounds for either perception in agricultural research.

Importantly, the historical rate of return on agricultural research investments provides valuable information relevant to the first possible justification for change. Although some assumptions underlying the various models can be challenged and some of the data used are disquietingly shallow, the high rate-of-return estimates are profoundly robust. Nearly all studies have estimated a historical rate of

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return to agricultural research investments in excess of the average return on industrial capital investments (Evenson, Waggoner, and Ruttan, p. 1103). This is true not only with regard to studies of aggregate U.S. agricultural research investments, but also U.S. regional investments, foreign investments, and commodity-specific investments. Consequently, unless equity costs of such research have been extremely high, the only plausible conclusion from the rate-of-return studies is that there has been general underinvestment in agricultural research. Some evidence exists that the rate of return on aggregate U.S. agricultural research investments may be decreasing with time (Peterson and Fitzharris, p. 78; Evenson, et al., p. 1103), but the most recent estimates are still in excess of 20% per year. Thus, underinvestment in agricultural research is still apparent.

With such high estimates of historical rates of return, it is difficult to argue logically that the seemingly loose ex ante evaluation procedures used in the past haven't worked well. So the first justification for arguing a need for change must be dismissed.

Arriving at a clear conclusion concerning the second justification is not so easy. It is a truism that we can always do better. Unfortunately that truism has little practical value. We are not working with optimum vs. suboptimum because in ex ante evaluation the optimum is indeterminate. There is simply too much uncertainty in the nonrepetitive new-knowledge production function. The primary issue in our evaluation of ex ante evaluation procedures is not whether some bad projects have been funded in the past or whether more will be in the future. It is fundamentally whether we will decrease the errors and increase the total payoff from research efforts by changing the evaluation procedure.

Arguments in favor of change often focus on the benefits of "systematic" evaluations and greater "objectivity." Many of the proposed alternative evaluation procedures are clearly systematic. They permit categorizing, ordering, comparing, and summarizing data in ways that are internally consistent and thus systematic. The question is whether they permit any greater objectivity than the evaluation methods used historically. This issue will be probed more deeply.

Role of Objective Data in Ex Ante Analysis

Objectivity is obviously preferred to subjectivity because decisions made objectively can be more easily defended via rational thought processes. It is easier to convince another person of the factual nature of objective than of subjective observations. Since the future is unknown and highly uncertain, the only objective data are historical observations. Because methods for measuring the historical research performance have received considerable attention, the use of such historical data is a logical place to begin. If it is possible to confidently correlate historical research performance with future research payoff at a very micro level, our evaluation procedures could introduce a measure of objectivism that would increase their administrative value considerably.

Within the profession, there is considerable expectation that a scientist who has proved to be highly productive in the past is a good risk for future investments of support funds. Although examples to the contrary abound, there is strong sentiment that historical performance of a scientist is a useful tool for predicting future productivity. Thus, funding agencies continue to invest a large share of their money with proven researchers. There appears to be little risk that investments in a Samuelson or a Friedman will not pay rich dividends.

Unfortunately, no research organization is made up of all Samuelsons and Friedmans. Few have even one. How then can these administrators take advantage of historical information to help them determine which additional research areas to promote and which to retrench from?

I have previously proposed a set of four sufficient conditions that would permit the use of historical information at the research area level in objectively predicting future payoff (Shumway, 1977, p. 192). The conditions are:
(1) proposed projects are competitive for available resources
(2) research technology used on each historical and proposed project is of comparable quality for its time
(3) the production function for new knowledge discovery is characterized by an S-shaped production function, and
(4) each project represents a small movement along the knowledge production function.

These four conditions are sufficient to establish an orderly relationship between past and future research payoff. I have made a defense for each condition, but the defense for conditions (3) and(4) remains the weakest. The likelihood that the new knowledge production function is anything close to a smooth S-shape seems quite low. A less restrictive concave production function would still be sufficient, but it would have to be reasonably smooth. It is the smoothness property that is most in question.

Because these conditions have not been tested as formal hypotheses, arguments that there is a more orderly than random relationship between past and future research payoff remain conjectural. Since originally proposing these conditions, my own confidence in their general satisfaction has diminished considerably. Based on casual but increasing amounts of empiricism, I would have to concede that the likelihood of all four conditions being met in all, or even most, of the applied research areas of any state or federal agricultural research unit is quite low.

It is true, for example, that breakthroughs in hybrid corn research have had important effects on hybrid grain sorghum and wheat research. But what has been the payoff from recent genetic research on corn? The marginal physical product of genetic research on corn could be characterized more as a few important blips (e.g., male sterile techniques, upright leaves, and high-lysine varieties) and one huge blimp (hybrids) than as anything close to a smooth function.

While hard scientific data on the subject are lacking, a few case studies document the need to be cautious not to overestimate the value of objective data for the ex ante funding decision. It appears unlikely that my sufficient conditions will prove very useful. Unless someone identifies another set with a considerably higher likelihood of being met, objective data will continue to play a minor role in ex ante research evaluation.

Many casual observers, including some research administrators and even some analysts, mistakenly attribute objectivism to certain evaluation techniques. The fallacy is in equating objectivity with quantifiability. Many techniques do use quantitative and/or qualitative inputs and provide quantitative evaluation outputs, but all ex ante research evaluation procedures are inherently subjective. The only difference is where subjectivity enters and how it is processed.

With Q-sort, subjectivity is imposed by the administrator at the highest level of abstraction in grouping projects into categories of similar perceived overall worth. With scoring models, subjectivity determines the specification and weighting of criteria and the categorization of each project relative to each criterion. Only the computation of the overall score proceeds in an "objective" fashion (Moore and Baker). With ex ante benefit-cost and rate-of-return estimates, subjectivity is also inherent in the estimates of both research benefits and investment costs (e.g., Araji et al). The predicted benefit-cost ratio and the rate-of-return estimate are quantified, but they are nonetheless subjectively based. Formalized optimization models (e.g., Shumway and Hwang) rely on subjective evaluations of the relative importance of objectives, expected achievement of objectives, probability of success, and expected cost. Even Rausser et al's recently proposed four-stage evaluation procedure is based almost exclusively on subjective data.

The only contributions any of the formal ex ante evaluation techniques can make are to (1) permit a formalization of the role of subjectivity, (2) suggest collection of objective information relevant to formation of subjective assessments, (3) insert subjectivity in forms where it is easiest for administrators and scientists or where the greatest confidence in its validity exists, and (4) process subjective data systematically to feed back information relevant to the funding decision. They cannot make objective outputs out of subjective inputs no matter how precise and elegant they may appear. Consequently, the legitimate role of subjectivity in ex ante evaluation needs to be clearly recognized and respected.

Assessment of Formal Ex Ante Evaluation Procedures

It is evident that many of the formal evaluation procedures are systematic. It is not clear they are in any sense more conducive of objectivity than the commonly used methods of evaluation. The fact that systematic evaluation procedures have been recommended to research administrators for at least 15 years and few have implemented them is strong prima facie evidence that their costs outweigh their perceived benefits.

By way of quaint but hopefully relevant comparison, if a car is broken, the mechanic tries to fix it. However, if it is running well and is getting better gas mileage and emits fewer pollutants than other comparable cars, he leaves it alone. He doesn't overhaul it until he has some evidence it can perform better than it is now.

The agricultural research establishment is not perfect, but it has performed well in the past without formal ex ante evaluation techniques. Further, there is no convincing evidence it will perform any better in the future with them. A heavy burden of proof that administrators need sophisticated and formalized ex ante research evaluation systems still rests upon the system developers.

Administration-Scientist Synergism

It is entirely possible that efforts in developing relevant evaluation techniques have focused on the wrong person. Administrators must make project selection decisions. But the alternatives they select from are formulated mainly by scientists, not by administrators. The organization must have proposals generated by imaginative, capable scientists who are well tuned to the problems of the public and the respective disciplines, or the administrators have only hollow project selection decisions to make.

A serious potential risk emanates from all the attention given to systematic and rigorous ex ante research evaluation. It is not impossible that greater administrative intervention in project selection will lead to submission of a larger number of proposals but with lower quality research being conducted on all of them. The larger number of proposals permits administrators to exercise their responsibility of decision-making. But, will the quality and quantity of research conducted by the organization be as great as when performed by well-motivated scientific entrepreneurs unencumbered by either the paper requirements or the annoyance of organizational demands for ex ante evaluation and ex post accountability? It may be that the best service the analysts can perform is to help administrators articulate relevant criteria to guide administration-scientist interaction so scientists can be more productive.

Unless the research organization is going to change scientists more often than any do now, the only relevant research alternatives are those that can be pursued by existing scientists plus a few new ones. Therefore, the appropriate place to begin the evaluation process is with the scientist. How can this entrepreneur be helped to select projects with high potential payoff?

Problem selection is generally the most important and most difficult part of inquiry. It is important because it delimits the range of investigation and establishes upper limits, although undefined, on the potential payoff of the inquiry. It is difficult largely because there are no formal rules that can be given by which scientists can learn to ask significant questions that lead to a perception and statement of significant problems. The ability to formulate important problems whose solution may also help solve other problems is often considered a rare gift.

Yet, some relevant guides can be identified to assist this learning process and sharpen the subjective perception of relevance. Sources of valuable signals must be cultivated, rational thought processes must be used in serious evaluation, and the subconscious must not be totally throttled.

Sources of Signals Concerning Research Priorities

Only a weak economic market exists for public research products since few products are sold and most are placed in the public domain at little or no charge to the user. However, an obvious source of research priority signals is still the market system. There is a strong economic market on the resource side of research, i.e., the job market. Bids and offers for particular scientists largely reflect a perception of the relevance, quality, and quantity of their work. While such offers are determined primarily by intermediate research products (i.e., publications), they are proxies for anticipated societal value of the final products.

Market signals also come from the user level in the form of legislative support or lack of support for research budgets. If the political processes work smoothly, legislators are going to reflect the attitudes of a majority of their constituents. Thus, the budget message is a type of economic signal from the end-product market. These signals in some form flow through the research administration. Administrative priorities consequently become proxies for public preferences.

While these economic signals are important, they are not sufficient for ex ante evaluation because they are either not sufficiently specific or they are based on assessments of work already completed. Additional sources of signals need to be cultivated.

The individual researcher needs communication linkages to determine current societal priorities. Hildreth and Castle (pp. 23-25) divide societal expressions of priorities into two forms: felt needs and gaps between goals and achievements. Both types of problems necessitate maintaining a finger on the societal pulse at the grassroots level. Because our clientele include all of society, it is difficult to maintain an accurate sense of needs and problems. However, we do have the infrastructure in place to pick up such signals from a large subset of our clientele if we will just use it. The Extension Service is charged with the responsibility of taking new knowledge discovered by agricultural researchers, packaging it in lay terminology, and delivering it to the relevant publics. Because they are in continual contact with the ultimate users of our research products, they are in an excellent position to also observe and listen to what those users think they want in terms of additional knowledge. Often that knowledge requires research, so their desires must be transmitted so that a research project can be conceptualized to provide answers to the issues raised. Strong two-way bridges of communication between research and extension must be developed to permit flow of research priority signals to researchers as well as new knowledge to users. Too often these communication bridges are weakened by real or fancied arrogance, indifference, or disdain.

The individual researcher also needs to develop communication linkages for anticipating future societal problems and preparing to help resolve them. It is the responsibility of the profession to address problems not yet faced and to build the theoretical structure and analytical tools to deal with them when they occur. Hildreth and Castle (pp. 25-26) also divide professional expressions of problems into two forms:

deviation from a theoretical optimum and intellectual difficulty. How many scientific breakthroughs have come because a scientist grappled with a perceived problem that didn't mesh with his theoretical scheme? How many profound discoveries have occurred because a scientist sought to untangle an intellectual paradox without any real perception of the possible practical ramifications of such an inquiry? Sources of signals to hone the scientist's perception of such theoretical research priorities include his own reading, attending professional meetings, and interacting with other scientists.

Of course, none of these sources of signals is sufficient alone. The scientist who has confined his entire attention to interaction with other scientists may find some applications of his theoretical work that could have highly fruitful consequences in resolving practical problems. Likewise, the researcher who is tuned only to current societal problems may greatly lengthen the long-term relevance of his work by studying associated theoretical and analytical developments.

Project Selection by the Scientist

By greasing the communication skids for problem identification, the scientist will always find himself faced with far more interesting and important research problems than he can possibly address. The next challenge is to establish his own set of priorities so he can propose those projects to the administration that he considers to have the highest payoff. It is unlikely that the quantitative evaluation models will be very useful to him, but a few simple questions may help him consider relevant issues. A set of potentially helpful questions for focusing one's thought processes includes the following:
(1) Who is your clientele?
(2) What are your priorities as to audience service (e.g., policymakers, researchers, farmers)?
(3) To whom is the problem important?
(4) To how many is the problem important?
(5) How much benefit will the clientele receive if this problem is solved?
(6) Do you have the analytical tools to conduct the research?
(7) What is the likelihood that your research effort will provide (or at least contribute to) a solution to the problem?
(8) What are the expected research costs (money and time)?
(9) What are the expected implementation costs?

No weights are suggested. Answers to these or similar questions help to identify weak links in the proposed study and promote communication between scientist and administrator. It is possible that his answers to questions 1 and 2 may be different than his administrator would like them to be, but explicitly defining them can be valuable in uncovering differences that could remain undefined and be a source of confusing interaction between the two.

"Artistic Considerations"

Because identification and pursuit of significant research problems is not an exact science, the role of nonobjective research tools also needs to be addressed. In a recent article, Ladd identified some of the most frequently used, versatile, and valuable research tools, none of which lend themselves to formal incorporation in a quantitative *ex ante* project evaluation model. They include the subconscious mental processes of imagination, intuition, and hunch, the unpredictable role of chance and serendipity, and the stimulating effects both on the subconscious and on research efficiency from writing. These artistic tools are probably at least as important to productive research as are the orderly and systematic rational thought processes. Consequently, whatever procedures are considered to assure *ex ante* evaluation and *ex post* accountability ought to be weighed carefully against any possible negative impact on these valuable research tools.

Conflicting Signals

One of the most difficult challenges facing administrators is the resolution of conflicts. Signals coming from different sources are not going to harmonize. The noise must be filtered in order to make order out of seeming confusion. Even with filtering, however, it would be a rare phenomenon to discover that all signals are directed to a single goal. What benefits one group typically hurts another.

Order suggests that a unifying goal should be pursued and all intermediate objectives should be clearly directed to the ultimate goal. However, research is an inherently uncertain production process. The search for new knowledge is laden with heavy risks and many dead ends. Consequently, it may be unwise for a research organization to establish a single overriding objective. Perhaps it should deliberately be a little schizophrenic and simultaneously pursue conflicting goals in order to be prepared for alternative conditions. For example, agricultural policy research in recent years has shifted from fundamental concern about managing surpluses to world hunger back to managing surpluses again. It is quite likely that world hunger will re-emerge as a major issue as soon as the political scene changes.

An experiment station cannot afford to concern itself exclusively with increasing production efficiency to the exclusion of marketing efficiency and equity. Any particular researcher may be single minded, but the organization must generally be concerned with a multiplicity of relevant objectives, many of which will be in conflict.

Conclusions

Four concluding recommendations are drawn from this attempt to elaborate the relevance of subjectivity in *ex ante* research evaluation:

(1) Let's be realistic about the contribution formal research evaluation techniques can make. No more should be promised than can be delivered. Quantification is not synonymous with "better." Negative impacts on scientist morale, ambition, and imagination must be weighed carefully against any expected benefits from increasing the planning, evaluation, and accountability functions of the organization.

(2) The prominent place of subjectivity in *ex ante* evaluation needs formal recognition and respect. Just as quantification does not mean better, neither does it imply objectivity. Any *ex ante* evaluation is intrinsically subjective. Objective historical observations may be relevant but the linkage between past and future knowledge generation is sufficiently weak to require gross subjective synthesis and assessment.

(3) Instead of worrying so much about dividing an existing pie among many competitive alternatives, let's concentrate on educating administrators how to relevantly use historical data. Historical rate-of-return estimates are sufficiently high and robust to imply that the major deficiency has not been in allocation but in level of overall investment. In seems clear there has been public underinvestment in agricultural research.

(4) The role of the individual scientist in *ex ante* evaluation warrants considerably more attention. Without his generation of ideas and aggressive pursuit of interesting problems, the research organization would stagnate regardless of the valiant efforts of the administration. He is the first and most important participant in the research evaluation process. He is fundamentally concerned with and involved in project selection. Evaluation techniques that fail to recognize that crucial linkage or demand additional energies from him in documentation and accountability for the system's sake are probably doomed to a dismal failure.

Footnotes

1/One of the reviewers suggested a third reason for proposing a change in *ex ante* research evaluation--budget appropriators insist on a change in the evaluation process before they will appropriate. Actually, this is merely a symptom that the appropriators have one of the two perceptions identified in the text.

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